

S.J.B

A. M. D. G.
BULLETIN
of the
American Association
of Jesuit Scientists
(Eastern Section)



Published at
LOYOLA COLLEGE
BALTIMORE, MARYLAND

VOL. XIII

MARCH, 1936

NO. 3

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Bulletin of American Association of Jesuit Scientists

EASTERN STATES DIVISION

VOL. XIII

MARCH 1936

No. 3

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EDITORIAL

A NATIONAL SCIENCE BULLETIN

On the following page we present a letter from the V. Rev. Daniel M. O'Connell, Commissioner of Education for the American Assistancy. In this communication his Reverence clearly states his views and the wishes of V. Rev. Father General. The purpose of this venture is also evident, since union and cooperation make for better government in the vast field of education. Great efforts are being made by our Superiors and Deans of Studies to comply with the wishes of V. Rev. Father General for this national union and cooperation "which he urges so strongly in the first four articles of the *instructio*". How are we to accomplish such coordination in science?

If we examine the Courses of Studies in our catalogs for undergraduate schools, we find that the sciences and mathematics are important features of college education and that more hours are given to science than anyone other subject. For students majoring in biology, there are four years given to this science; for chemistry, there are four or five one-year courses; for physics, there are four courses; and for mathematics, there are four courses. Not one of all the other subjects occupies the same amount of time, whether it be metaphysics, psychology, ethics, English, history, ancient literature, apologetics, economics, modern languages or art.

We can have better cooperation among our Teachers of science by annual conventions and by a National Science Bulletin.

What is the educational value of a Science Journal? We may suggest: the coordination of science to philosophy and philosophy to science; the exposition of scientific theories for class-room lectures; methods for improving pedagogical problems; recording scientific research problems; improved methods for laboratory work; suggesting outstanding books helpful to teachers; the work of scientific clubs and extra-curriculum activities, obituaries of outstanding Jesuit scientists and their publications.

Is the BULLETIN fulfilling this purpose? Is the BULLETIN an educational magazine?—If we examine the recent issue we find: an article treating the subject of CAUSALITY, an important issue in combating modern materialistic philosophy; these articles are being used in one of Our Houses of Studies and in the Seminar in one

(Continued on page 113)

A COMMUNICATION FROM
V. REV. DANIEL M. O'CONNELL, S.J.

LOYOLA UNIVERSITY
6525 Sheridan Road
Chicago, Illinois

Rev. R. B. Schmitt, S.J.,
Loyola College,
Baltimore, Maryland.

Dear Father Schmitt:—

P.C.

I thank you for the December issue of the Bulletin of the American Association of Jesuit Scientists.

I have had an opportunity to read it carefully. I am very proud of it and I feel that I should tell you so, and through you, when you have the opportunity, all who contributed to it. Each article is deserving of publication.

There is a pleasing variety of subjects, too: Science and Philosophy, Astronomy, Biology, Chemistry, Mathematics, Meteorology, Physics and Seismology.

I am glad to see that you have Correspondents from other Provinces. His Paternity will bless this national union and cooperation, which he urges so strongly in the first four articles of the *Instructio*. I trust that we shall soon have a national Jesuit convention of Scientists and Mathematicians. In the meantime, I am sure that you will welcome contributions from and through your Correspondents. I take it for granted that every school of Ours subscribes to your excellent Bulletin.

May the coming year of 1936 be most happy for you and the work you are doing *Ad Majorem Dei Gloriam*.

If I can be of any help to you and your cause at any time, please call on me.

Sincerely in Christ,

DANIEL M. O'CONNELL, S.J.,
Commissioner of Education.

SCIENCE AND PHILOSOPHY

NATURE'S LAWS AND THE PRINCIPLE OF CAUSALITY

PART II—CAUSALITY AND STATISTICAL LAWS

REV. JOSEPH P. KELLY, S.J.

In classical Physics, one of the chief assets of a Law of Nature from the scientific point of view, was that it enabled the scientist to predict experimental results. While admitting the validity, at least in theory, of the distinction placed by Planck (1), between a causal process and predictability, as a criterion for judging it, yet we believe that, in science, causality and predictability became practically identical. For it is held that what cannot be observed or what cannot be expressed in quantitative terms, is practically non-existent. "There is no need to assume the existence of that which cannot be perceived," says Einstein. Hence, because the Michelson-Morley experiments showed a negative result, i.e., there appeared no quantitative value for the "ether-drift", this eminent Physicist denied that the ether existed. If the scientist, 'ex-professo', confines his efforts to the limits of the observable phenomena of Nature and to these, insofar as they can be expressed by symbols in a mathematical formula, he is, in principle, logical when he denies the 'scientific existence' of non-observables or non-quantitative elements. By means of a law of Nature, the Physicist and Chemist is able to predict the results of an experiment, provided, of course, that he has sufficient knowledge of certain antecedent factors. For this is one of the crucial tests of a theory, that it can foretell certain experimental results and that these predictions be verified by actual data. The history of Relativity offers a good example of this scientific attitude of mind. A non-predictable event is, generally speaking, of no particular value in science. In practise, then predictability and causality become one and the same thing, due to the fact that causality in science is 'ante-factum' and previsionsal and because of the pragmatic value of predictable results (2).

Although a law of Nature is a generalization, a sort of general principle, yet in its application it deals with individuals. Physical bodies used to be considered as individual, essentially homogeneous units. Their natural activity was attributed to the body as a whole;

(1) cf. Bulletin, Dec. 1935. p. 57

(2) cf. Bulletin, Dec. 1935. p. 58.

properties belonged to the body as such. The Physical Law which states that a metal rod expands in a definite manner, (according to the coefficient of expansion) under the influence of heat, describes a definite effect produced in a body by the activity of another upon the first. The fact of expansion is independent of any theory of the composition of matter or any other theory of heat. Heat causes metals to expand. This 'cause' is explained by saying that it is the nature of heat to produce this effect; that it cannot act in any other way under these circumstances. This is called Physical Determinism, or as some call it, Natural Causation.

In the application of these Laws, it is difficult to obtain an experimental result precisely equal to the calculated effect. It is impossible to completely eliminate all instrumental errors and accidental errors on the part of the observer. The law itself is expressed in such a way that it often omits elements which must of necessity enter into an actual event. The physical formula $S = \frac{1}{2}gT^2$ in itself does not take into account air resistance. In other words, material bodies do not fall to the ground, according to that formula, with mathematical exactness. Hence, the values obtained by these laws are approximations of the first degree.

Statistical Laws

The Atomic Theory of Matter and the Kinetic Theory of gases introduced a new outlook on Nature's activities and the working of the Laws of Nature. These theories lead us directly to a "particle-world" in place of the "bulk-world" which was essential to classical ideas. It is held that in a gas, for example, the molecules are in a state of constant agitation. They move in this direction or that, apparently at random. They strike against each other and against the sides of the vessel which contains them. Hence the notion of pressure assumes a new guise. It is no longer the mass of a substance pressing against the walls of the container but the result of a "constant bombardment of molecules against the walls of the containing vessel." Boyle and Bernoulli gave voice to these notions in the 17th. and 18th. centuries but Clausius is to be considered as the real founder of the kinetic theory of gases. Apparently, then, there is a constant pressure exerted against the walls of the container; it is at all points and in all directions. In this theory the pressure is the sum of all the impacts of the molecules. Since these impacts are irregular, the pressure must vary and if we could closely examine a small area for a brief interval of time, we could perhaps note the variations. However, so rapid and numerous are the collisions against the sides of the containing vessel that neither our senses nor our instruments are able to detect an appreciable variation. Hence Wulf concludes: "The conception of a gas pressure being occasioned by a rapid succession of molecular impacts is *not contradicted by experience*." (3)

(3) Wulf. "Modern Physics." p. 117.

The kinetic theory alters our notion of a law of Nature. For, in this case we are not dealing with an individual phenomenon as such but with a "group-phenomenon"; we are considering an average result and have therefore a Statistical Law. According to this law the scientist may tell what has happened or what will happen with respect to a group of molecules in certain conditions, but the law does not apply to individuals as such. We cannot predict with certainty the outcome of an individual molecule. In a given field of activity, we observe a large number of events, or the action of a large group of molecules. They seem to act independently of one another and with a great deal of irregularity. In spite of this, the scientist observes a mode of behavior that repeats itself with some constancy *in the group*. There is an *average behavior* for the "group-phenomenon." The Statistical Law of nature express this average result. "The law which governs this new phenomenon states that, on account of the almost infinitely large number of 'elementary' events, the number of impacts can, at no time or place, differ appreciably from the average value. But one realizes quite clearly that this certainty is very different than when we deduce a law from experimental observation only. In such case, whether or not an exception will be found, depends upon the fineness of our senses, or the delicacy of our instruments. Small deviations will occur quite frequently, but the greater a deviation is the less frequent will be its occurrence. And even if an appreciable discrepancy should be noticed on some occasion, when the usual instruments are being employed, that would not invalidate the meaning of the law. In place of certainty, a statistical law is really an expression of probability. 'It is improbable' that, for any one complete second, the number of impacts made at any one point on the wall should differ considerably from the normal number. The number of impacts made in different seconds will never be exactly the same, but large deviations which are so large in magnitude, and which last for such a time, that they could be perceived by us, occur so rarely, that actually nobody has ever observed them." (4)

It would seem that this concept of a Statistical Law of Nature conflicts sharply with Physical Determinism which was so fundamental in Classical Physics. Determinism seems to be relegated to the realm of "less necessary concepts." Yet it cannot become useless. Pure chance does not find a permanent footing in science. This has produced something of a crisis in scientific thought and the scientist finds himself in this paradoxical situation. The rapid development and success of the physical sciences has been based in large measure on the validity of a Physical Determinism. True, the principle was pragmatic and had "its sanction in the fact that it worked." As a working principle it was eminently advantageous, in fact, so much so, that it became a real foundation stone for the experimental sciences.

(4) Wulf. "Modern Physics," p. 118.

Science cannot reject it outright, for that would be equivalent to admitting that the progress of two centuries was founded on a false principle. On the other hand, a sort of "indeterminism" seem to belong to the nature of a Statistical Law. Many scientists hesitate to reject so easily a principle which has proved itself to be of prime importance in the natural sciences. Hence, they look for some way out of the dilemma. They say that the indeterminism means this: the laws of Nature which were so well applied to large bodies, to the Macrocosmic order, cannot be thus applied to the phenomena of the Atomic world; or that it represents the problem of the "limits of application" of dynamic Physical Laws. Others believe it to mean that it is now impossible to interpret a "single quantum in an atomic process" in terms of mechanical laws. Dirac concludes that strict causality is definitely excluded from Physics. "Since Physics is concerned only with observable magnitudes, the classical theory of determinism is indefensible. . . . the disturbances which the observer causes in a system under observation are directly under his control and are acts of his free will." (5) Eddington has frequently professed this same opinion in his public lectures. Planck, on the contrary, seems to insist on a background of strict causality in order to justify the validity of statistical laws. According to classical laws of science prediction of results were made with a high degree of mathematical accuracy. The concrete conditions of the experiment, lack of precision of instruments, outside influences and the personal equation could easily explain the slight error between the actual findings of an experiment and the advance calculations. But prediction demanded an anterior, determined cause.

Statistical Laws are, in some respects, similar to the Classical Laws. They are generalizations of experience, and they also offer to the scientist a means of looking to future results. But whereas the dynamic laws pointed to individual action, the statistical laws tell of "group-action". Sometimes, it is true, they try to describe the action of an individual in the group. In this case they either leave the individual undesignated or else indicate only the probability of its action. As for example, we might say that *some one molecule* out of a possible thousand will strike the point A, or again it is *only probable* that the molecule, M, will strike the point A. For, since we are dealing with groups as groups, the average result will permit variations in specific instances without destroying the validity of the Statistical Law. We can then say, that in a given second of time, a certain number of Molecules will strike a definite area (supposing now a determined experiment), but we can say only with a probability that any given molecule will do so. None will deny the utility of statistical laws in predicting average results with a satisfactory degree of accuracy. What is the reason why the scientist is able to follow the same process of prediction here as in the classical Laws? If it is

(5) Memoire Au Congrès Solvay. 1927.

predicted that the average span of life is, e.g., 60 years, in a certain country among a certain race, that may represent merely what has happened over a period of years. It is not a law; it is simply a calculation of past events. Suppose it is considered as a law, according to which events will take place in the future, has it some underlying guarantee of determinism? Certainly; for it supposes that human bodies will be subject to and be affected by the physical conditions of climate, sanitation, etc., which have an influence on the general health of that community. In other words, we may consider statistical laws as a *second degree of approximations*, since they afford a solid basis for predicting group results instead of individual results. In this case, however, it supposes that there are definite and determined factors, although unknown in detail to the observer, which in given conditions will produce fixed results; unless there be some such solid basis to statistics, we would never be able to elevate them to the perfection of a law of nature, nor be able to use them for the calculation of future events. They would remain as mere summaries of past experience, a grouping of haphazard events, of no real value to science. The guarantee of a statistical law lies in the objective determinism of certain individual factors. "The fact that we have statistical laws, is dependent on the assumption of the strict law of causality functioning in each case. Our lack of knowledge or lack of data may prevent us from applying the principle of causality but that does not at all mean that the principle has failed." (6)

We must bear in mind that a causal process which cannot be recognized and described is equivalently non-existing in science. The criterion for recognition, according to Planck, is the capability of predicting accurately the results of this process. Here precisely is the problem in Statistical Laws which offer us at most a probability. Accurate prediction becomes impossible because we have not sufficient knowledge of antecedent factors. If we accept the opinion of Wulf, . . . "what cannot be proved, as knowledge now stands, is the existence of the electronic orbits around the nucleus, as postulated by Bohr. Moreover, we can never learn at what definite time an electron will be at certain point in an orbit." (7) This being so, the present state of our knowledge prevents us from predicting the future state of the electron at a given moment. Hence, there follows the logical impossibility of determining the simultaneous position and momentum of an electron. That a given electron will, as a matter of fact, have a definite position and momentum at a stated time, I believe, the scientist would admit. But it is one thing to say that this is so and quite another to say that we cannot have accurate knowledge of it. Just as there is a great difference between asserting that a causal process cannot be proved and that a phenomenon actually happens in a causal manner independently of our knowledge of it. An insufficient knowl-

(6) Planck. "Where is Science Going." p. 145.

(7) Wulf. "Modern Physics." p. 463.

edge may, indeed, prevent the scientist from predicting a determined result, but that does not mean that the result, as it exists in nature is not determined. Supposing, then, that causality, in its scientific bearing "looks only to the future", and under this aspect alone it is judged, it may be quite logical, from this view-point to deny causality in certain cases.

How are we to judge this tendency on the part of science to reject causality? The problem has a distinctly epistemological aspect, for the concept has undergone some radical changes in the past three centuries. It began with Galileo and his followers, when they clothed physical explanations with mathematical robes; when they replaced natural philosophy with experimental data and quantitative interpretations of nature's phenomena. Not that these two points of view are necessarily exclusive, but in the heat of discussion the human mind can with difficulty maintain the distinction between the two view-points. The early leaders of quantitative science certainly admitted the validity of philosophical concepts, while perceiving readily the value of mathematics in physical explanations, as well as the progress to be derived from this method. The experimental method offered them an independence, as it were, of metaphysics. The fact that a body fell to the ground, with a certain velocity and acceleration, could be satisfactorily expressed in numerical terms without recourse to the "natural positions" of Aristotle. Newton's Laws of Motion and the "force of gravity" did not require the "intelligent spirits" for an interpretation of the course of the heavenly bodies in their orbits. As the experimental sciences developed without relying directly on metaphysics, it was only natural that philosophical notions were neglected or pushed farther and farther into the background; the next step was the elimination of these concepts as completely as possible from mathematical formulae. Many fell into the error of denying all validity to metaphysical principles, but this false step is now recognized. The fact that many of our present day scientists are seeking a metaphysic for science, is at least a part vindication for the necessity of certain universal principles as a foundation for all human thought.

Let us consider the problem of causality in the light of the advancement of positive science. In the previous article, we noted the modifications that were given to the concept of cause, due to the emphasis placed on the observable and measurable qualities of physical objects. (8). The very nature and success of the experimental method has created an empirical or spatio-temporal vocabulary, so to speak, in contradistinction to the philosophical. As, for example, I may describe differently two pieces of iron according to their size, shape, color, hardness, etc. Yet the chemist implicitly recognizes in them, a common factor, a constant among all the variables; a permanent

(8) cf. "Bulletin," Dec. 1935.

quality amid the "hic et nunc", changeable notes. This constant we call the nature or substance. It is not directly observable nor measurable but is the underlying substratum of all the observable qualities of any physical being. Since it cannot be, "in directo," the object of any experimental method, the scientist considers it outside his scope. He is therefore content with those observables which will describe for him this or that particular object. The tendency of the positive sciences has been to build up this experimental edifice and consequently to substitute as far as possible quantitative definitions in place of philosophical concepts; to represent by a mathematical formula nature's phenomena than to interpret them in terms of metaphysics. That this goal can be ultimately attained, viz. a complete isolation of science from philosophy, is I believe a very doubtful proposition. In view of the limitations of the natural sciences and the method to which the scientists have committed themselves, we can say that this "rejection" of causality represents primarily a further step in the advance of the experimental method. In other words, the scientist, using the statistical method need no longer to have recourse to a causality concept. Not that the causal process has been disproved, nor that it is an invalid concept in other fields of human knowledge, but what we call a causal factor does not fall within the quantitative and mathematical interpretation of nature. The point of emphasis is a new independence—so to speak—of a metaphysical notion that was a necessary supposition in Classical Physics. From what has been explained above we can see that the inability to predict and accurate result, plays an important role in this new outlook.

Does the scientist deny causality in statistical laws? In one sense, he does. In reality he has rejected his own definition, a definition formed under the prevailing influence of observables and measurables and fitted especially to the experimental methods. He renounces a concept of causality, which is limited to "looking to the future" and whose value is essentially bound up with predictability. As we said above, there is no one who denies to the scientist the liberty of proposing new definitions or modifying concepts, consistently with his fundamental principles or postulates. But it must be remembered that any subsequent affirmation or rejection of these will have an effect *only in the field of these newly-formed definitions*. It can in no way affect the validity of these same notions in branches of knowledge outside the positive sciences. Hence we can say that there is no denial of the traditional concept of a cause nor of the principle of causality as we have explained it in the first article of this series. That this notion retains its validity in science, is gathered from the fact that no scientist would admit that a physical event is *uncaused*, if regarded from a "post-factum" point of view. The source of difficulty in the interpretation of the present problem is that causality in science has become "ante-factum" and previsional. In conclusion we

may say that the traditional notion of causality in itself, still remains untouched by the statistical question. A rational justification of Statistical Laws and their actual use in science must presuppose the validity of natural causality in the minute events which compose the statistical phenomenon. Traditional causality has not failed even though we may not be able to prove it in detail. It is one thing to assert that two events are causally connected and to be able to give a reason why that statement is true. It is quite a difficult matter to be able to represent in mathematical or quantitative terms, the antecedent of two events and from this to predict the consequent result.



EDITORIAL

A NATIONAL SCIENCE BULLETIN

(Continued)

of Our colleges in a post-graduate course. We also find a record of the brilliant research of one of Ours, who invented the most accurate clock ever made: the Free-Pendulum clock, applicable in astronomy and seismology. For the biologist, there is an article treating *Blood Transfusions*, a scientific study for an important medical problem. In chemistry, we have a description of the most recent development in analytical chemistry for the micro determination of bromine and chlorine in organic substances, particularly applicable in the synthesis of hormones. In mathematics, there is a pedagogical article discussing the problem in the Freshman classes in colleges. In meteorology, there is a record of the research work being done at the Manila Observatory on "Clouds and Air-currents". For the Physics Professors, we find a new development of conditioning sun-light, and an excellent list of books. There is an announcement of the seismological work of the Society being extended to Kingston, Jamaica, B. W. I. We find too: a list of publications of our colleges and universities; recognition by the Pope of the excellent scientific work of the Jesuits at the Vatican Observatory; a record of the non-resident lecturers at one of Our colleges; a description of the Jesuit Memorial Museum at Santa Clara. All of these are most assuredly *educational* and worthy of publication and mutual help among our Jesuit educators. Several thousand reprints have been made of articles in the recent issues.

If this is the result of a small group of Ours doing work in science, what an excellent publication could be had, if all the seven provinces in the United States were coordinated and published a National Science Bulletin.

R. B. S.

ASTRONOMY

GEORGETOWN OBSERVATORY EXPEDITION TO STUDY SUN'S ECLIPSE IN ASIA

REV. PAUL A. McNALLY, S.J.

A joint expedition to observe the next total eclipse of the sun—scheduled to sweep across Asia on June 19, 1936—will be sent to Soviet Russia by Georgetown University and the National Geographic Society.

The expedition will travel halfway around the earth to make observations during the brief two and one-half minutes when the moon will come between the earth and the sun and temporarily turn day into night. Even so brief an observation of the sun is considered well worth while by astronomers because it gives them a rare opportunity to study the sun's corona—a halo of pearly light extending hundreds of thousands of miles outward from the sun but visible only during an eclipse when the rest of the sun's light is cut off.

Dr. Paul A. McNally, S.J., of the Georgetown College Observatory, will be leader of the expedition, accompanied by five others to be chosen from the staffs of the University and The Society. They will leave sometime in April and return in July. Observations will be made from a point near Orenburg, Soviet Russia, because past weather records show that this region offers one of the best promises of clear weather along the whole path of the eclipse.

Headquarters of the expedition will be established near Orenburg, probably at the village of Sara, which is very near the line along which the center of the moon's shadow will travel. Orenburg is about 775 miles southeast of Moscow.

This eclipse, first total eclipse of the sun to be visible on earth since that of February, 1934, will begin at sunrise in the Mediterranean Sea off the southwestern coast of the Grecian Peloponnesus. The moon's shadow, marking a path of totality about 50 miles wide, will sweep in a direction north of east across the Aegean Sea, Istanbul (Constantinople), and the Black Sea, will pass south of Rostov and Stalingrad, across Orenburg, and over Omsk and Tomsk in Siberia.

Moving over the northern tip of Lake Baikal, the path will curve south of east, over the northern portion of Manchutikuo (Manchu-

kuo) and then over Khabarovsk and the northern portion of the Japanese Island of Hokkaido. Reaching Hokkaido late in the afternoon, the moon's shadow will pass out to sea and the eclipse will end in the Pacific Ocean a few hundred miles to the east at sunset.

The Governments of both Soviet Russia and Japan have extended cordial invitations to the scientific organizations of the world to send expeditions to their territories for observation of the eclipse.

Dr. McNally was the director of an expedition in 1932, sent by Georgetown University to Maine for the total eclipse of the sun observable there and obtained valuable experience in the unusually rapid photographic work which must be carried on during the brief period of darkness. The 1936 expedition will probably make use of a battery of four or five special cameras mounted so as to follow the apparent motion of the sun, photometers for making studies of changes in light intensity, spectrographs to study the sun's light, and instruments to note weather changes during the eclipse. The chief photographic objective will be the obtaining of pictures of the sun's corona.

The corona, which can be seen extending in vast streamers of pearly light around the edges of the sun when the black disk of the moon moves in front of it, is believed to be superheated vapor rising from the seething surface of the sun. Recently astronomers have found good evidence that it consists largely of oxygen, the life-supporting gas of the earth's own atmosphere.

Photographs taken during the eclipse, timed with great exactness, will give astronomers a chance to "hold a stop watch" on the movements of the solar system and see if it is "running on schedule." Movements of the sun, moon and planets in relation to one another are predicted with extreme accuracy by astronomers, but the prediction can be checked only when two heavenly bodies pass each other, as in the case of an eclipse.



BIOLOGY

GENIC BALANCE, SEX DETERMINATION AND SELECTIVE FERTILIZATION IN HABROBRACON.

REV. CHARLES A. BERGER, S.J.

For some years Dr. P. W. Whiting and others have been working on the problem of sex determination in the parasitic wasp *Habrobracon juglandis*. Three papers appeared during July of this year summing up the facts ascertained and proposing theories to explain them.

The article by Whiting in the Jn. of Heredity for July 1933 (1) is a rather complete account of the whole investigation. The paper in the Proc. of the Am. Phil. Soc., is a very much condensed summary of the results. (2) The paper by Dr. G. D. Snell (3) in the Proc. of the Nat. Acad. of Scs., is controversial; he analyses the facts found by Whiting and others, accepts Whiting's main theory but rejects one of the subsidiary theories, namely that of Selective Fertilization, and proposes a simpler explanation. In the Jn. of Heredity for Jan., 1935, Whiting presents a good summary of the whole disputed question of Selective Fertilization (4).

In the wasp *Habrobracon* fertilized eggs develop into females (diploid) and unfertilized eggs develop parthenogenetically into males (haploid).

This type of sex-determination, females developing from fertilized eggs and males from unfertilized eggs is common throughout the entire order *Hymenoptera*; it is also found in many Rotifers and some other forms.

It had been quite generally taken for granted that in these organisms both of the two sets of chromosomes in the diploid female were identical with the single set present in the male. This assumption was quite irreconcilable with the theory of 'Genic Balance' proposed by Bridges and established by genetic investigation during the past ten years.

The theory of 'Genic Balance' as applied to sex-determination may be briefly stated as follows: "Sex is determined by a balance or ratio of sex-genes, some of which are in the sex-chromosomes, others in the autosomes, some tending to produce femaleness, others tending toward maleness."

The best evidence for genic balance has come from *Drosophila* in which many intersexual and supersexual forms have been found associated with a change in the ratio of autosomes to sex-chromosomes. The normal wild type ratios and some of the more important abnormal ones are as follows:

AA + XX = normal ♀	AA + XY = normal ♂
AAA + XXX = " ♀	AAA + XY = super - ♂
AAAA + XXXX = " ♀	
AAA + XX = intersex	AA + XXY = ♀
AA + XXX = super - ♀	AA + X = ♂ (sterile)

Therefore in *Drosophila*:

- 1) sex-genes in the X tend towards ♀
- 2) " " " " A (autosomes) tend toward ♂
- 3) Y takes no part in sex-determination but has a gene for male fertility.

Obviously the condition in *Habrobracon* could not be reconciled with the principal of Genic Balance. If the male wasp has the constitution (A+X) and the female is (AA+XX) the ratio of sex-chromosomes to autosomes is the same in both sexes and they should be the same on the principles of Genic Balance.

Some authors tried to solve the problem by supposing that Genic Balance does not apply in the case of the *Hymenoptera*, that sex-determination in that order of insects was a quantitative not a qualitative process; i.e., a double dose of sex genes gives a female while a single dose gives a male.

This solution had to be abandoned when it was shown that diploid males are occasionally produced from fertilized eggs.

In *Habrobracon* Whiting established the fact that when closely related individuals are mated a small percentage of diploid males are produced from fertilized eggs. The percentage of these biparental males produced among the total of all biparentals (males and females) varied from 1—25% of the total biparental progeny, being apparently a function of the closeness of relationship of the parents. When individuals from different and unrelated stocks were crossed no biparental males appeared.

Whiting proposes the following theory of sex-determination which at the same time fits the facts in *Habrobracon* and is consonant with the principle of Genic Balance:

Females have two sets of autosomes (AA) plus two different sex-chromosomes (X & Y), hence they are digametic and produce two kinds of unfertilized eggs and consequently two kinds of parthenogenetic males (A+X) and (A+Y) in equal numbers.

These two types of males are phenotypically indistinguishable.

The X and the Y chromosome are assumed to differ in the following way:

The X is assumed to contain one or more dominant genes (F)

necessary for the production of a female but incapable of producing a female by itself.

The Y is assumed to have another different factor or group of factors (G) also necessary for the production of a female but incapable of producing one by itself.

The female sex is determined by the complimentary action of (F) and (G) when both the X and the Y chromosome are present in the same fertilized egg.

Either X or Y alone gives a haploid male.

Either XX or YY in a fertilized egg gives a biparental (diploid) male.

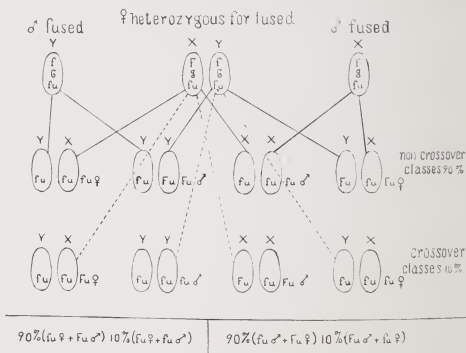
Up to the present all efforts to identify the sex-chromosomes cytologically in *Habrobracon* have failed, hence Whiting was forced to use genetic experiments to prove his theory that the female has two kinds of sex chromosomes (X & Y) and that the haploid males were of two kinds, one having an X, the other a Y.

He gives two proofs; the first from the action of a sex-linked recessive mutation called *fused* (*fu*), causing the fusion of the segments of the antennae and of some of the mouth parts:

When *fused* (*fu*) males were crossed with females heterozygous for *fused* (*fu/Fp*) the results from a number of pair matings always fell into one of two classes: either:

- a) *fused* females and normal biparental males made up much more than one half of the total biparental progeny
- or b) the same two class made up much less than one half the biparental progeny.

By following out a diagram of the above crosses we can readily see how these results are explained on Whiting's theory. (Fig. 1).



These experiments show:

1) That 'fused' is sex-linked, with 10% crossing-over between the (fu) and the (F/G) loci.

2) That females are (XY) otherwise there is no explanation of the two sexes in the biparental progeny.

3) That males are X or Y, shown by the reciprocal results of different matings.

Whiting's second proof is had from the occasional finding of haploid, left-right mosaic males which are partial sex-intergrades. These males are found among the parthenogenetic sons of females heterozygous for some mutant gene or genes.

They show some mutant characters on one side of the body not on the other, indicating that there are two kinds of haploid male tissue, that on one side containing an X chromosome and that on the other containing the Y. These mosaic males show a feminization of the male genitalia which Whiting explains in the following way: the X bearing tissue one side of the body elaborates and sends out a diffusible substance, the Y bearing tissue on the other side does likewise, in the mid-line of the body these two diffusing substances meet and by their complementary action bring about the feminization observed in the male reproductive organs.

Thus far in the argument Snell is in perfect agreement with Whiting and he gives Whiting credit for having solved the sex-determination problem in *Habrobracon* by a simple unified theory that is in accord with the principle of Genic Balance.

One of the observed facts is however not explained by Whiting's theory, i.e., the relative infrequency in the appearance of biparental males. According to the theory as outlined above fertilized eggs should be formed in the following classes and proportions: 1XX : 2 XY : 1 YY, i.e., females and biparental males should be formed in equal numbers. This is never the case. In outbreeding no biparental males are formed; in inbreeding from 1-25% of biparental males among the total biparental progeny are formed, depending on the closeness of the relationship of the parents.

Whiting says that undoubtedly 'Selective Mortality' is a partial cause of this lack of biparental males, but equally undoubtedly it is not the adequate cause of such wide and constant variations in frequency as occur (i.e., ranging from complete absence in outcrossing to from 1-25% in inbreeding).

As the only possible solution Whiting introduces a subsidiary hypothesis of Selective Fertilization. In brief it is this: either type of sperm (X or Y) may enter the egg. If an X sperm enters differential maturation takes place, X bearing polar bodies are thrown off and a Y bearing egg nucleus is retained as the female pronucleus. If a Y sperm enters differential maturation also takes place but re-

sults in the retention of an X bearing egg nucleus as the female pronucleus.

This hypothesis explains but it is a pure postulate, unsupported by other evidence and the whole idea of selective fertilization is in rather bad scientific standing.

Snell disagrees with Whiting on the necessity of assuming any selective fertilization or differential maturation to account for the relative infrequency of biparental males. He proposes the following theory which is merely an extension of Whiting's main theory:

Femaleness in *Habrobracon* instead of being due to heterozygosity in a single pair of sex factors (X & Y), is determined by heterozygosity in one or more of several pairs of sex factors, some of which are in different chromosomes. A scientific precedent for this condition is had in *Drosophila* and in *Lebistes*.

The several pairs of sex factors may be represented thus X/Y-W/Z-M/N-O/P-Q/R.

1) As long as a fertilized egg is heterozygous for any one or more of these factors it develops into a female.

2) Only when the fertilized egg is homozygous for all of its sex factors does it develop into a male.

On this supposition it is obvious that there will be many more possible combinations in which at least one of these factors is heterozygous than there are possible combinations in which all are homozygous. This according to Snell is the main cause of the relative infrequency of biparental males.

The different results of inbreeding and outbreeding are also explained by Snell's theory since it is a commonplace of Genetics that outbreeding tends to increase heterozygosity (which means higher per cent of females) and inbreeding tends to increase homozygosity which in our case means a higher percentage of biparental males.

(1) Sex Determination in Bees & Wasps.—P. W. Whiting, Jn. of Hered. 26, 7, July 1935.

(2) Genic Balance, Sex Determination & Selective Fertilization in Hymenoptera.—P. W. Whiting, Proc. Am. Phil. Soc. 75, 6, 1935.

(3) The Determination of Sex in *Habrobracon*.—G. D. Snell, Proc. Nat. Ac. Scs. 21, 7, July 1935.

(4) Selective Fertilization.—P. W. Whiting, Jn. of Hered. 26, 1, Jan. 1935.



INHERITANCE OF BLOOD-GROUPS

REV. CLARENCE E. SHAFFREY, S.J.

The observations of von Dungern and Herszfeld have yielded this first and fundamental result: the biochemical structures of A and B never make their appearance in the offspring unless they were present in one or both of the parents. If both parents possess a peculiar structure it will also be found as a rule in their children, though in certain cases it may be wanting. If the father or mother only has

the structure, it is usually found in some only of the children, though exceptionally it may occur in all of them. If, on the other hand the structure is wanting in both parents, it will also be absent in the children.

This means that if groups A, B, or AB are present in the father or the mother, they may or may not be transmitted to the children; if both parents belong to group O, their offspring will invariably belong to this group. An analysis of the statistical data obtained by these writers convinced them that the transmission of the agglutinogens in the blood obeyed Mendel's law, and that all possible contingencies in the transmission of blood-groups from parents to children could be completely explained in this manner.

Since we have seen that properties A and B of the red corpuscles may fail to appear in the children, whereas they do not under any circumstances make a *de novo* appearance in them, we must obviously, on Mendelian principles, be dealing with characters which, if they are present in the blood at all, must be manifest; in other words, they must be Mendelian dominants. On the other hand, the absence of these characters which may be observed in the children in contrast to the parents, shows all the signs of a Mendelian recessive. We may note that the absence of the characters A and B is not merely negative, for in the adult at any rate, it is accompanied by the presence of the agglutinins a and b in the serum.

An explanation of this fact thus brought to light was tentatively put forward by von Dungern and Herszfeld in agreement with orthodox Mendelian principles. They suggested that the blood-groups might be the resultant of four characters united in two allelomorphic pairs: A and a, B and b. Note here that a and b do not in this discussion, namely the inheritance of blood-groups, stand for or indicate what they did in the previous pages. There a and b stood for agglutinins, here they indicate absence of the character and have nothing to do with agglutinins. Hence a means 'not A' and b means 'not B'. These two pairs would be completely independent of each other and would be situated far apart in the chromosome system; thus, for example, Aa might be equivalent to black and white, and Bb might be equivalent to tall and short.

It is clear, or will be from the following diagrams, that the four blood-groups can be formed by the combination of two pairs of allelomorphic characters; for each pair gives rise to three genotypes, and if dominance is complete, to two phenotypes only. Thus:

B	b	B	b	A	a	A	a
BB	Bb	Bb	bb	AA	Aa	Aa	aa
BB=dominant homozygote				AA=dominant homozygote			
Bb=dominant heterozygote				Aa=dominant heterozygote			
bb=recessive homozygote				aa=recessive homozygote			
First two of Phenotype B				First two of phenotype A			

The combination of the two pairs gives rise to 9 genotypes, thus:

	aa	Aa	AA	From the diagram we deduce:
	aa	Aa	AA	Group O=aabb
bB	aa	Aa	AA	" A=AAbb Aabb
	aa	Aa	AA	" B=aaBB aaBb
Bb	Bb	Bb	Bb	" AB=AABB AaBB
	aa	Aa	AA	AABb AaBb
BB	BB	BB	BB	

Hence we would have the four groups or phenotypes from the combination of the two allelomorphic pairs of characters. However, there are some difficulties in the way of such an explanation which we shall see a little later. To avoid these difficulties and present a theory more in accordance with the facts, Bernstein, a mathematician, in 1925 sought to explain the inheritance of blood-groups on the basis of two characters only, one from the father and one from the mother, which may be dominant or recessive, the same or different, according to the following scheme, which shows the relationship between the phenotypes (blood-groups) and the genotypes. Bernstein claims that the hypothesis of three multiple allelomorphs combining with each other 2 by 2 and located in the same part of the chromosome (similar to what was found by Morgan to be the case in *Drosophila*) agrees much more closely with the facts. These 3 allelomorphs which correspond to three different games, as opposed to 4 of von Dungern and Herszfeld, are named by Bernstein A, B and R. Blood-Groups and Hereditary Types. (Bernstein)

Groups	O	A	B	AB	
Hereditary	RR	AA	BB	AB	R is the recessive.
Formula		AR	BR		
	A	B	R		
A	AA	AB	AR		
B	AB	RB	BR		From the diagram we have one
R	AR	BR	RR		of O, to two of AB, to three of
					A, to three of B.

In 1928 K. H. Bauer suggested the assumption of two pairs of partially linked factors, instead of triple allelomorphs, as the basis of inheritance of blood groups. Bauer claims his hypothesis explains exceptions occurring in certain cases or crosses. He assumed 11 per cent of cross-overs between the linked factors. Bauer's hypothesis is accepted in two recent text-books: Gates, "Heredity in Man", p. 193, and Castle's 4th revised edition of "Genetics and Eugenics", p. 372. Bauer's reason for his hypothesis is that in crosses of groups O and AB, only children A and B should be obtained, whereas, occasionally children of Groups O and AB have been reported from such matings. Snyder rejects Bauer's hypothesis for two reasons:

1. Not at all certain that exceptions do occur to the hypothesis of triple allelomorphs.

2. If such exceptions do exist, linkage will not explain them.

Development of the Refutation

On the basis of triple allelomorphs, the genotypes of the 4 blood groups are:

Group O—OO
“ A—AO, AA
“ B—BB, BO
“ AB—AB

Crosses of O and AB should give only children of groups A and B, thus:

O	O	A	B
OA	OB	OA	OB

Since O is the recessive we would have only A and B children. Actually some children have been reported as of group O or group AB from matings of O and AB parents, but most of these exceptions were reported between 1910 and 1925 before grouping technique was standardized. Since 1925 very few exceptions have been noted.

MULTIPLE ALLELOMORPHISM AS OPPOSED LINKAGE IN BLOOD GROUP HEREDITY

PROF. LAURENCE H. SNYDER

Dept. of Zoology, Ohio State U. Am. Naturalist Vol. 65, 1931

The inheritance of the human blood groups has been shown to be dependent upon a set of triple allelomorphs (Bernstein, 1925, Snyder, 1926, 1929, et al.). Based on this interpretation, laws relating to legal and clinical medicine, and to anthropology have been formulated (Snyder). Former suggestions of hypotheses, involving: 1. 2 pairs of independent factors; and 2. 2 pairs of completely linked factors, have been shown to be untenable.

In 1928 K. H. Bauer suggested the assumption of 2 pairs of partially linked factors, instead of triple allelomorphs, as the basis of inheritance of blood groups. Bauer claims his hypothesis explains exceptions occurring in certain crosses. He assumed 11% cross over between the two linked factors.

Bauer's hypothesis of linkage is accepted in two recent text books: Gates, "Heredity in Man" (page 196) and Castle, Fourth Revised Edition of "Genetics and Eugenics" p. 372.

Bauer's reason for advancing his hypothesis—in crosses of O and AB, only children of A and B should be obtained, whereas, occasional children of groups of O and AB have been recorded from such matings. The hypothesis of linked factors purports to explain this.

REFUTATION OF LINKAGE HYPOTHESIS

1. Not at all certain that exceptions do occur to the hypothesis of triple allelomorphs.

2. If some such exceptions do exist, linkage will not explain them.

DEVELOPMENT OF REFUTATION

On the basis of triple allelomorphs, the genotypes of the four blood groups are:

Group O—OO
Group A—AO, AA
Group B—BB, BO
Group AB—AB

Crosses of O × AB should give only children of groups A and B:

OO × AB

AO, BO

Actually, some children of groups O and AB have been recorded from such matings. But most of these exceptions occurred (1910-1925) before grouping technique was standardized. Since 1925 very few exceptions have been noted.

Table 1

OFFSPRING OF CROSSES OF GROUPS O AND AB

	Numbers in Group				% Exceptions
	O	A	B	AB	
1. Before hypothesis of triple al- lelomorphs	31	94	61	26	36.7
2. Since hypothesis of triple al- lelomorphs	14	405	400	7	
3. With known errors removed					
2	7	401	399	5	1.5

Reason for 3.—of the 14 exceptional Group O children, 2 were illegitimate, 4 were wrongly included because of the mixing of two families of the same name. Same for 2 of the 7 unexpected AB children.

N.B. With these removed, the percentage of exceptions is only 1.5, and this in spite of the fact that several investigators have concentrated on this type of mating with the express purpose of looking for exceptions. The small remaining percent. may be explained on the basis of illegitimacy, mistaken in technique, mixing of babies in hospitals, or on the basis of nondisjunction.

Matings of Groups O and AB are hard to obtain since Group AB is rare. Groups of mothers and babies in hospitals were taken:

OFFSPRING OF GROUP AB MOTHERS

Number of families	Number in Group			
	A	A	B	AB
371	2	196	150	175

Therefore, there are two exceptions, since AB children are possible in as much as the father could be of any of the 4 groups. exceptions, and he found two exceptions out of seven cases.

CONCLUSION

The exceptions to the hypothesis of triple allelomorphs are not numerous.

Looking at Table 1 it might seem that the second and third columns might represent non-cross overs, and the first and fourth cross overs. This is how Bauer interpreted them, so also Castle and Gates.

All three of these workers apparently lost sight of the fact that in a random mating population the coupling and repulsion phases are not conveniently separated as they are in cages of experimental animals, but that both phases will occur, *and in equal proportions* even assuming only a small percentage of crossing over. Time is the only requisite. This would mean that the four columns in Table 1 should be approximately equal on the basis of linkage. Complete equality would never occur because of the presence of some single and double homozygotes among the Group AB individuals.

Let us assume that blood groups are inherited as linked factors. The genotypes of the four factors would be:

Group O—(ab) (ab)

Group A—(Ab) (Ab), (Ab) (ab)

Group B—(aB) (aB), (aB) (ab)

Group AB—(AB) (AB), (AB) (Ab), (AB) (aB),
(AB) (ab), (Ab) (aB)

Bauer apparently assumed that Group AB, resulting from the combinations of Groups A and B, would be only of the formula (Ab) (aB). If this were true, the results in Table 1 could properly be interpreted as cross overs and non-cross overs. The assumption is that the original phase was the repulsion phase, i.e., that A and B were linked on one chromosome and a & B on the other. This condition would not prevail long, unless the factors were completely linked, and no further mutations take place. (But Bauer establishes that there is 11% crossing over).

If any crossing over at all occurred, the new combination (AB) would be formed. Since this would be as stable, once it was formed, as either of the original combinations (aB) or (Ab) it would only occasionally cross back when the opportunity was presented, that is, in combination with a chromosome carrying (ab). The new combina-

tion (AB) would thus tend to pile up and it is this fact which makes the hypothesis of linked factors untenable. (This seems weak to me.)

The deficiency of Group AB in comparison with expectations, is proved by a statistical analysis and so it is demonstrated that linkage, which would give us the expected but, in fact, unrealized ratios does not hold.

Levine has suggested the possibility of non-disjunction as an explanation of exceptional cases. This seems to be the most reasonable explanation of exceptions if they are proved to exist.

CONCLUSION

Which theory is the better explanation of the known facts, von Dungern's and Herszfeld's or Bernstein's? Both theories agree in this: "No agglutinogens can appear in the children which were not present in one or other of the parents." When, however, one of the parents belongs to group AB, the two theories no longer agree. In this case according to von Dungern and Herszfeld, the children may belong to any group, while according to Bernstein, *they cannot belong to group O*. Moreover, in the special case of combination of O and AB it is clear *they cannot belong to group AB* either.

TABLE SUMMARIZING RESULTS OF INVESTIGATIONS BY MANY
REPUTABLE AUTHORITIES

Parental Combination	No. of Families	Number of Children in Group.			
		O	A	B	AB
O x O	1192	2630	15	2	—
A x A	1256	476	2364	1	1
A x O	2535	2256	3021	18	9
B x B	293	126	—	532	1
B x O	997	958	11	1230	1
A x B	1104	401	791	641	580
AB x O	465	38	571	525	34
AB x A	481	21	525	253	307
AB x B	327	13	121	306	159
AB x AB	67	—	39	42	70
Total	8717	6919	7458	3550	1162

The underscored numbers are exceptions to expectations. These figures confirm the following: 1. The blood-group in an inheritable property. 2. It is transmitted in accordance with Mendel's law, through two independent, dominant characters, agglutinogens A and B. All agree on this. While there seem to be a few exceptions to the rule they are very few, since out of 8500 families with about 19,000 children, there are only 30 to 40 discrepant families with just over 60 children. The discrepancies may be due to illegitimacies, or errors in blood determination.

HEREDITY OF THE AGGLUTINOGENS M AND N

In 1928 Landsteiner and Levine discovered these two agglutinogens, M and N. They found that the agglutinogens cannot appear in the children unless present in one or both of the parents. Human blood forms no anti-bodies for M and N, that is there are no corresponding agglutinins. After much research, Dr. Eisler of Vienna developed a useful test-serum from the blood of rabbits. But this serum could be preserved for only a short time, and hence it was not practical, for instance in connection with court proceedings. Eisler has since produced a serum that can be preserved for at least a year, and is suitable for transportation. This serum can be used in medico-legal cases, as for instance in the determination of non-paternity. These three new groups are now added to the previously known blood-groups. Comprehensive examinations on more than 1000 families and more than 3000 children have shown that these agglutinogens are inherited according to Mendelian laws. If the blood group M or N is present in the blood of the parents, it will be found without exceptions in the blood of the children of that union. It is, for example impossible that a child with the M blood-group should have parents with the N blood-group. In that case therefore in connection with court proceedings to establish the paternity of a child, a father of the N group can be excluded. The determination of the M and N groups alone is sufficient to divert suspicion from a suspected or supposed father in 18 per cent of the cases. In combination with the A and B groups, the number of positive results is increased to 31 per cent.

See Diagrams 12 and 15 for the method of exclusion, using M and N.

See Diagram 13 for method of exclusion by means of three multiple allelomorphs.

See method of exclusion by means of sub-groups in Table 12, Tallant's Th.

INHERITANCE OF THE SUB-GROUPS

Landsteiner and Levine (1926) said that these sub-groups were due to the existence of two sub-groups of the agglutininogen A, which they designated A_1 and A_2 . They suggested that the sub-groups of group A be designated as above, and those of group AB be designated A_1B and A_2B .

Thomsen, Friedenreich and Worsane (1930) proposed a theory of the heredity of the sub-groups. Instead of the three allelomorphous genes as formulated by Bernstein, they said four allelomorphous genes exist, namely, A_1 , A_2 , B and R. Where A_1 , A_2 , and B are dominant over R, and A_1 is dominant over A_2 . According to this theory there would exist six blood-groups, the genotypes and phenotypes of which are as seen in Diagram 11.

Friedenreich and Zacho (1931) examined 103 families and found no exceptions to Thomsen's theory. However, this is not a sufficiently large survey to warrant absolute acceptance of the theory.

CHEMISTRY

STUDENT GRADES IN QUANTITATIVE ANALYSIS

REV. FRANCIS W. POWER, S.J.

While it is comparatively easy to grade students' examination papers, it is often hard to decide what mark to give in the case of reports of laboratory work in quantitative analysis. This article is intended to be of some help along this line.

As it would be rather unfair to the student to expect him to duplicate the analytical work of experienced chemists, one should rather attempt to find some more appropriate norm, and this I take to be the work of the students themselves. For some years I have kept track of the agreement of the students' reported results with the "theoretical" analysis for the samples given them, and have taken the average discrepancy (in parts per 1000 of the constituent being determined) between the reported result of the student and the "theoretical" result to represent an average mark, say 75. Perfect agreement would, of course, indicate a mark of 100, and about twice the average discrepancy may be given 60. If the discrepancy is greater than this the student is told to repeat the analysis. The average discrepancy which appears in Table II for each of the determinations listed is the mean of all the reported results of anywhere from 50 to 200 students, depending on how many of them ran the determination in question. At Fordham only a few students take the full year course in quantitative analysis involving gravimetric determinations; most of them take only a one semester course which is concerned only with volumetric procedures.

The determinations done by the students are as follows:

Standardization of N/10 HCl via AgCl

Determination of Na_2CO_3 in soda ash

Standardization of N/10 NaOH against the standard acid

Determination of the strength of an unknown acid

Standardization of N/10 permanganate via sodium oxalate

Determination of Fe in an iron ore (Zimmermann-Reinhardt method)

Determination of MnO_2 in pyrolusite (reducing with sodium oxalate)

Standardization of N/10 thiosulphate via KIO_3 or KBrO_3

Determination of Cu in a crude copper oxide (iodimetrically)
 Standardization of N/10 iodine via pure arsenious oxide
 Determination of As in a crude arsenious oxide

The students who continue for the full course (2 semesters) then go on with the following:

Standardization of N/10 AgNO_3 solution (Volhard's method)
 Determination of Cl in a crude chloride sample (Volhard's method)
 Determination of S in a sulphate by precipitation as BaSO_4
 Determination of insolubles, oxides, CaO , MgO and CO_2 in a limestone
 Determination of Sn, Pb, Cu and Zn in brass, the copper run electrolytically

The course calls for 2 lectures a week, and two 2 hour laboratory periods a week. Theoretically there should be 30 lectures and 30 laboratory periods per semester, but various holidays, etc., cut this figure down to 24 or 25; and some of these lecture periods, of course, have to be used for examinations. However, by careful planning of lectures one can get in the bare outline of analytical theory, including problems and a little about hydrogen-ion concentration. Also, by considerable driving one can get the students through all the determinations listed; I find that due to initial unfamiliarity with the use of balance and to the various ingenious complications which the students can introduce into such simple determinations, it will take them the first quarter (up to about the middle of November) to carry out the acid-base experiments. The oxidation-reduction reactions keep them exceedingly busy during the second quarter, up to about the third week in January. The quarter mark is the 50-50 average of the examination grade and the laboratory grade.

It might be of interest to record the distribution of marks for the course for the past three years. The marks given in the following table for the final term (or year) are marks for 221 students:

TABLE I

Final Mark	Percentage of the class getting these marks
41-50	1.3%
51-60	7.3
61-70	25.0
71-80	31.9
81-90	30.0
91-100	4.5

Before giving the table of marks corresponding to student precision it will be well to warn any one who proposes to use this data that they are based on reports on "unknowns" most of which have been carefully checked by some one of the staff. It is not fair to the

student to ask him to check his results against any Tom, Dick and Harry who may call himself an analyst. Most of the samples I have purchased from the various companies who supply this sort of material have analyzed very close to the values assigned them, but one set of sodium carbonates had four samples nowhere near what they were supposed to be, and once in a while some other sample turns up whose analysis is not all that it might be. When one has a small class and can get along on a correspondingly small number of samples it should be possible for the professor to run every sample himself; where there are many samples, at least a few from each lot should be run so as to get some assurance that there is nothing seriously wrong with them. I keep about 100 quantitative unknowns on the shelves, and have standard acid, 0.01 N, 0.1N, 0.5 N, and normal on hand, besides 0.1 N, 0.5 N, 1.0 normal base, 0.1 N permanganate, ceric sulphate, ferrous ammonium sulphate, silver nitrate, potassium thiocyanate, sodium thiosulphate, and iodine, each solution standardized carefully from time to time as need arises for "umpire work". If a student who has usually turned in good results reports a differing widely from the "theoretical" value of the sample, I look up the sample to see if I have run it myself; if I have not and an examination of the student's work shows no great source of error on his part, I run the sample at once, holding the student's mark in abeyance. If his figure agrees with mine he is right—if not, he's wrong! Somebody has to settle it, but I think it fairer to the student to give him the benefit of the doubt and judge him according to my own results which he can see done rather than tell him flatly that he is wrong when I do not *know* whether he is or not.

Incidentally, the student's results are obtained on uncalibrated burettes and weights. I suppose this is unscientific, but I tolerate this practice for the following reasons: (1) the calibration would take so long that the student would become thoroughly disgusted with this tedious and uninteresting work and far more practical and interesting analytical work would have to be omitted for lack of time; (2) with all due respect to the abilities of the young men I feel sure that the "corrections" they would introduce would do far more harm to their volumes and weights than would the uncorrected errors in the apparatus; since (3) I have known only one instance, about 10 years ago, where such errors amounted to more than a couple of parts per thousand anyway. Weights and glassware purchased on specification from reputable dealers are quite accurate enough for student purposes without calibration in my opinion; an isolated case may arise where this becomes the chief source of error, but as I say I have only met one such case—that of an old imported pre-war burette, which was 7 cc. off at the bottom! We kept this burette around the laboratory for a long time as a horrible example, but it has since been lost.

Table II

Student Grades In Quantitative Analysis Based On Average Student Performance												
DETERMINATIONS		Na ₂ CO ₃	Acid	Iron	MnO ₂	As ₂ O ₃	Cl	S	CO ₂	SiO ₂	CaO	MgO
Average Discrepancy in parts per 1000		7	11	8	7	6	7 15% to 40% Cl	12 8% S	18	40	23	150 2% to 10% MgO
GRADES		Discrepancies in parts per 1000 from "true" results										
95		1.5	2.0	1.5	2.0	0.5	1.5	2.0	3.5	6	3	25
90		2.5	3.5	2.5	3.0	1.5	2.5	4.0	7.0	13	7	50
85		4.0	5.5	4.5	4.0	2.5	4.0	6.0	11.0	20	13	75
80		5.5	7.5	6.0	6.0	3.5	5.5	8.5	14.5	29	17	100
75		7.5	10.0	8.0	8.0	5.0	7.5	11.0	18.0	40	23	135
70		9.5	12.0	10.0	10.0	7.0	9.5	13.5	21.5	50	30	165
65		11.5	14.0	12.0	12.0	9.5	11.5	16.5	25.0	60	37	200
60		14.0	16.5	15.0	15.0	11.5	14.0	19.5	29.0	70	45	250

This table and method of grading was presented to the Division of Chemical Education at the New York meeting this spring. A tabulation of some student results (but without a grading system) was published by Buehrer and Schupp in the *Journal of Chemical Education*, 3, 1271 and 1421 (1926).

It is not possible to make any categorical statements about agreement or duplicates. Naturally if a student hands in a result that is satisfactory as regards checking the "theoretical" value but which is an average of duplicates that differ widely, he is invited to repeat the determination. One's own experience has to be the guide in this matter.



RECENT ADVANCES IN CHEMISTRY

REV. RICHARD B. SCHMITT, S.J.

As the new year is ushered in, we naturally look back to survey the progress that has been made in theoretical and industrial chemistry. The enormous amount of current literature lists the aggregate of endeavor in progress to discover the unknown. Some of the research problems are purely theoretical, some are immediate steps to proving hypotheses, and some are practical to industry, to more comfortable existence and to alleviate human suffering.

We wish to enumerate briefly the outstanding progress in chemistry during the past twelve-months period. The ultimate constitution of matter still occupies a prominent part in physico-chemical research and much data has been recorded on atomic structure, the properties of radiation and methods of controlling the neutron. Closely related to this type of research, we have developments in the field of transmutation of the elements by bombardment and artificial radio activity.

The hormones and vitamins also occupy an important place in research laboratories and efforts are being made to synthesize these vital molecules. In 1934 Dr. L. Ruzicka of Switzerland converted cholesterol into androsterone. This substance was isolated by Dr. Butenandt of Germany and it proved to be a male hormone. It has been definitely determined that there are at least two male hormones. Dr. Laqueur of the Netherlands claimed that he isolated the gland hormone and changed it by oxidation into androsterone.

After twenty-five years of persistent research Dr. R. R. Williams of the Bell Telephone Research Laboratories, claims to have found the exact chemical structure of vitamin B₁—part of what is

known as the vitamin B complex. A new synthesis makes possible large quantities of vitamin C, and corn is the raw material from which it is made. Dr. G. Wald of Harvard University found that vitamin A was one of the substances that participated in the process of chemical changes associated with the process of vision; this vitamin is found in the retina of the eye. Vitamin C and B are now available on the market in crystalline form.

Heavy hydrogen is produced on a larger and more economical scale, and so is available for more intensive research. A long list of experiments have been reported on the replacement of ordinary hydrogen with heavy hydrogen in many chemical compounds. Dr. Hugh Taylor of Princeton University is using heavy hydrogen, with nickel as a catalyst, to control the carbon-carbon and carbon-hydrogen bond in organic synthesis. In the same laboratories heavy hydrogen of mass 3 was isolated. In physiological processes heavy hydrogen was used as a "tracer" to determine how the body disposes of food materials.

Dr. Gustav of Berlin, reported a method of concentrating heavy neon. New isotopes were discovered, and now almost all known elements have isotopes, which means that the previously determined atomic weights are variables depending upon the isotopes of different masses present in the element.

Industrial chemistry made phenomenal advances during the past year. More than four hundred new chemical compounds were produced and marketed by the manufacturers. Among these are synthetic camphor, urea, plastics and new applications of plastics, lacquers and pigments. Surface tension control gave improved methods to the textile industry.

In photography, the outstanding development has been the commercial perfection of making motion pictures in natural colors on films into which color filters are placed by the use of multiple layers of emulsion. The pictures are photographed directly on the multiple layer film; during development by processes of selective action of the chemical developers and selective bleaching and dyeing of the several layers of the emulsion, the film is produced in natural colors.

The march of time is the march of chemical advance.



MATHEMATICS

THE CONCEPT OF ORDER

REV. FREDERICK W. SOHON, S.J.

The importance of the concept order will hardly be questioned by anyone. In fact we should say off hand that the difference between a scientific and an unscientific method of procedure may be in the concrete largely a question of order. The spectral lines of hydrogen no longer appear just a nasty mess when the neat little formula for the Balmer series has been called to our attention. We have a law. Order and law appear to be closely connected notions. Nor is the question of order of importance in the physical sciences alone. The philosophers have much to say upon the subject of order, and the mathematicians in their own way have given the question of order a fundamental place in the structure of their analysis.

There would probably have been no occasion for this paper if the notion of order were free from ambiguity. But the word order does not seem to be used always in the same sense, and if such is the case we should like to know what we mean when we use the word. I recall two definitions from my study of philosophy. The first defines order as "an arrangement of things according to some relation existing among them". The second definition I learned was that order was the "conjunction of many in the attainment of a common end". Are these two definitions convertible? In mathematics we speak of linear and of cyclic order, and these terms are not confined to geometrical applications. We also speak of the order of performing successive operations, the order of performing differentiations sometimes affecting the result, the order of applying rotations to a figure usually giving totally different results. We also speak of the order of a differential equation, the order of an infinitesimal, and of orders of infinitude. I do not intend to go into all these questions, but I merely cite them to bring out the point that when a person speaks of order we should not be too sure that we know just what he has in mind to convey by his choice of the word.

Is it possible to give a good definition of order in its most generic sense? By that I mean a definition that involves only notions that are simpler than the notion of order itself. For purpose of speculation it is not necessary to assume that this is possible. One

can grant that the term may be equivocal and try to set up a well defined concept simply to see how far its extension goes. This does not, of course, solve the problem, but it prepares the way. We should also remind ourselves that there are such things as correlative notions. Such notions are of such a nature that each immediately implies the existence of the other, so that each necessarily occurs in the definition of the other. This does not mean, as some seem to think, that neither can be defined, but it means that both must necessarily be defined together and the same identical statement expresses at the same time the definition of both. On the other hand it must be admitted that if such a statement merely points out the correlation and nothing more, then neither concept is defined. In so far, however, as the proposed definition adds additional information to the mere fact of correlation, in just so far the proposed definition contributes something to the definition of the notions involved.

Are the notions of order and of relationship such a pair of correlatives? Or are they merely different aspects of the same thing? Could it be that one and the same notion is referred to as order when considered in broad perspective, and is termed relationship when viewed close up and in detail? We have seen that the term relation occurs in one definition of order. What is a relation? *To esse ad*. What does the preposition mean? It appears merely to signify order. Is the notion of order more or less fundamental than the notion of relationship? It might appear that in its application—though not in its implication—the notion of relationship is simpler than the notion of order. On the other hand we speak with equal facility of complex relationships and of complex order.

Let us turn our attention to the opposites of the notions that we are considering. What are unrelated entities, and what is disorder? Here at least there appears some difference at least on the surface. But we must remember that statements that in their grammatical structure are contradictory assertions can be simultaneously true if they are made about ill defined concepts. Is disorder the contradictory of order, or is disorder a peculiar kind of order? Can there be any such things as unrelated entities? In space? The entire science of geometry protests. In time? Obviously not. But perhaps we beg the question with our prepositions. You will concede at least that the notion of unrelated entities—I mean totally unrelated entities—is a very difficult one. Perhaps it is even metaphysically repugnant. It is also clear that whenever unrelated entities are referred to, one never means to assert that the things in question are totally unrelated, but that one merely means that the relations that he expected to find—the relevant relations—are missing. Thus things may be both related and unrelated at the same time, depending upon our norm as to what relations are relevant to the matter in hand. This makes the notion of relationship more difficult than ever.

In a similar way, disorder would appear to be disagreement with an expected norm of order. In the most disorderly room there is still sufficient order for the propositions of geometry to be valid, in the most disorderly historical sequence there is still chronological order. Is there such a thing as a complete absence of order? Perhaps, perhaps not in the concrete, but in the abstract, perhaps the inferiors of some of our universal concepts may be said to be without order among themselves, not as they exist in actual reality but as they exist in the philosopher's ideal order. It seems certain that true absence of order can never occur in the concrete.

We seem to have forgotten the alternative definition of order that was cited in the earlier part of this paper, "the conjunction of many in the attainment of a common end". This is undoubtedly a useful notion but it implies a good deal more than the concept of "the arrangement of things according to some relation existing between them". I think that without undue violence to language we may speak of the former notion as formal order and the latter notion as mere material order. We feel sure that there is no such thing as material disorder. May there be formal disorder? Again the answer is not simple. Formal disorder may mean merely the disagreement with an expected norm of formal order, so that formal disorder again becomes a relative epithet implying an external and extrinsic criterion before it can be applied to or denied to a given concrete case. As for absolute formal disorder, the Christian philosopher will deny that it can exist because he believes in an intelligent first cause of all things. This, however, introduces another factor into the discussion. We had better return to our examination of mere material order.

In a discussion of mere material order in its relation to physical and mathematical law, it is a natural step to consider that order can be studied in various degrees of complexity. The extreme cases are the easiest to study, but the study of order of intermediate degree of complexity is not so simple. The dynamics of a particle with the laws of Kelper and Newton are not too much to expect the ordinary college student to be able to grasp. The dynamics of a system of particles furnishes intellectual exercise for students of graduate caliber with Lagrange and Hamilton and Jacobi lending a helping hand. Then let the system become vastly more complicated so that these powerful general methods can no longer be of service. What do we get? Absolute chaos? No, we get statistical science, Gibbs, the phase rule, physical chemistry, thermodynamics. There is no material disorder. Chaos exists only in our minds when we cannot grasp the problem.

We might pause here to inquire the philosophical implication of our contention that there is no such thing as material disorder. Have we destroyed the argument from design? If there is no essential

difference between simple order and chance—both being kinds of material order, what about the argument that order cannot come about by chance? Chance is ordered, and gives rise to order. **I**f the philosopher's argument just so much twaddle? The philosopher, although from the examples he uses he seems to be talking about mere material order, is really thinking about formal order. As we should say, his argument is not an argument from order but from design. If his correspondent, misled by some of the examples proposed to him, is solely occupied by the notion of material order, no wonder the argument appears to be inconclusive. If the mere existence of material order is proved, nothing is proved. The philosopher must prove the existence of the kind of order he has in mind. He must prove formal order. The notion of design is his middle term, if he is establishing the intelligence of the first cause. If he is merely trying to establish the existence, and not the intelligence of the first cause, he can confine his attention to material order—which proves nothing to the point—but the contingency of the existing material order. Thus in denying that there is any such thing as material disorder we have not disarmed the philosopher, but we have merely pointed out to him that his spear is not his breastplate though both are armaments, and that if he distinguished between them he would be more effective in combat with his adversaries.

It may be useful to point out that the laws of chance are real laws, and the study of statistics is a real science. The principles of this science are unfortunately not part of the common intuitions of all men. One should not assume that because he can read numbers he can interpret statistical information, or that if he gets a numerical result it has any meaning before he has investigated the probable error of his observations. This can be done because there is order in the realms of chance. There is order everywhere. There is no such thing as material disorder.

What then is order, and what are relations? Can they really be defined? Even if we cannot formulate an entirely satisfactory definition we should not despair because an unambiguous set of properties will serve for all practical purposes as a sufficient logical substitute for such a definition. Where disagreement arises concerning the properties to be marshalled for this purpose, the parties to the disagreement have different concepts in mind. Those properties which both agree to include serve logically to delineate a logical genus, and the properties concerning which the parties are at variance describe logically the specific difference between the two types of order in the minds of the two protagonists.



METEOROLOGY

THE MANILA OBSERVATORY AND THE INAUGURAL FLIGHT OF THE "CHINA CLIPPER"

REV. WILLIAM C. REPETTI, S.J.

The "China Clipper" alighted on Manila Bay at 3:32 P. M., Friday, November 29th, 1935. It had left Guam at 4:00 A. M. (Manila Time) and could have landed in Manila at 1:00 P. M. because of a favorable tail wind. Weather reports from the Philippine stations were furnished the Pan American officials on the 28th and 29th.

On Sunday, December 1st, Mr. Groeger, Pan-American operations manager, brought Captains Musick and Sullivan to the Observatory for a short visit to consult about the weather for the return trip to Guam.

In the afternoon the Observatory staff, with the exception of Father Selga who was still absent, visited the China Clipper and was cordially received by Mr. Bixby, Far East Pan-American representative, Mr. Groeger, Mr. Russell, airport engineer, Captain Musick and others of the Pan American personnel.

The following appreciation appeared in the "Herald" of November 30th.

"The Clipper, he observed, will somehow get beside atmospheric hazards: In this connection, Captain Musick praised the Manila Weather Bureau for its excellent reports furnished the Clipper during its flight to the Philippines. Paul Groeger, operations manager of the Pan-American in the Far East, kept in touch with the Weather Bureau during the flight, and gave frequent reports by radio to Captain Musick and his men."

Just before the Fathers left the Observatory for their visit to the Clipper a report came in from Truk Island, 540 miles southeast of Guam, indicating the presence of a typhoon near the island. This report was immediately taken to the Clipper.

The take-off for the return trip to Guam was at 3:00 A. M., Monday, December 2nd. This was three hours ahead of schedule but there were several reasons for it. To avoid a crowd of boats at the start, to arrive in Guam by daylight against expected head winds and to get there before the new typhoon approached dangerously near Guam.

The tentative plan for the cooperation of the Weather Bureau is to assign numbers to the meteorological stations in the eastern part of the Philippines from Aparri to Surigao. This will avoid loss of time and possible confusion in the transmission of long names. Weather conditions at these stations will be sent to the Pan-American at the time of flights or the planes may ask for a report of conditions at specified places. The entrance to the Philippines will depend on local conditions. The plane may come around from the north via Cape Bojeador or from the south via San Bernadino straits or even further south.

Emergency landings may be made at Lake Paoay in Ilocos Norte or at Cebu or Iloilo or at Cagayan.

At certain times of the year when the mornings are clear and the afternoons cloudy at Manila the planes will probably leave Guam in the evening, arrive at Manila in the morning and go on to Macao the same day.

December 3: The China Clipper took off from Manila at 3.00 A. M., yesterday morning and arrived in Guam at 4:40 Manila time; averaging about 115 miles per hour. The typhoon is still south of Guam. The Clipper was to hop to Wake Island to-day. No danger from the typhoon.

PAN-AMERICAN AIRWAYS COMPANY
TEMPORARY OFFICE
MANILA HOTEL
MANILA, P.I.

December 6th, 1935.

Manila Observatory,
406 Padre Faura,
Manila.

Attention—Father Doucette

Gentlemen:

On behalf of Pan-American Airways, Captain Musick, Navigation Officer Noonan and the flight crew, we wish to thank you and the members of your staff for the most important part that you played in the successful flight of the first trans-Pacific air mail flight.

Captain Musick told us that he had heard much about the Manila Observatory before visiting Manila but he carried away with him an impression even higher than that which he had previously entertained.

Yours Very Truly,

(Signed) HAROLD M. BIXBY.

HAROLD M. BIXBY,
Far Eastern Representative
Pan American Airways Company.

PHYSICS

THE POSITRON—ITS CREATION AND ANNIHILATION

REV. JOHN S. O'CONOR, S.J.

PART I

The discovery of the positive electron, like so many other scientific discoveries, was the by-product of a piece of research directed not at the uncovering of new building stones of matter, but rather at the elucidation of one of the most powerful and yet one of the most obscure sources of energy,—cosmic ray phenomena.

Carl D. Anderson of the California Institute of Technology secured photographs, in the Norman Bridge Laboratory, of the tracks of charged particles traversing his cloud chamber; and the circumstances of ionization, radius of curvature, direction, association with other tracks and loss of energy in passing through matter, were such that the conclusion reached, after exhausting all other possibilities, was that these tracks indicated the existence of a "positively charged particle comparable in mass and magnitude of charge with an electron." (1)

Discovered on August 2d., 1933, and reported September 1st. to Science; a more complete discussion with reproduction of the photographs, appeared later in the Physical Review. (2)

The necessity of such a conclusion can be best understood by a consideration of the relations existing between track curvature, magnetic field intensity, direction of motion and ionization density, of the track of a charged particle moving in a Wilson cloud chamber.

It has been deduced theoretically and proved experimentally that for very fast particles the ionization of the track is practically independent of the mass and depends only on the charge and velocity of the particle.

If the cloud chamber be pervaded with a magnetic field of intensity H then due to the deflecting force of this field acting at right angles to the motion of the particle, the path will become curved at right angles to the motion of the particle, the path will become curved with a radius of curvature which will be called r . The product Hr

is a function of both mass and velocity, the relations being given by the equation:

$$\frac{V}{C} = \frac{Hr}{\left[\left(\frac{mc}{e} \right)^2 + (Hr)^2 \right]^{1/2}}$$

where v =particle velocity; c =the velocity of light; m =the mass and e =the charge on the particle. If we consider only the elementary charge e , we see from the above relations that two particles with the same " Hr " but different masses will ionize differently, and it also follows of course that particles with the same " Hr " and the same mass will give the same ionization density.

Since there have been found *fast* particles of both positive and negative sign giving approximately the same ionization, it could correctly be concluded that the *charge* on both was of the same order of magnitude, since their velocities were the same,—(having been determined from their identical curvatures in the same magnetic field).

As particle tracks made by entities with the same magnitude of charge had been found, and these tracks had the same " Hr " within the limits of accuracy determinable in the cloud chamber, the conclusion that these entities had both the same magnitude of charge *and mass* was not only justified but inevitable.

If the direction of motion of the particles is known their signs can be readily deduced from the direction of curvature of their path due to their motion in the magnetic field. In Anderson's cloud chamber a lead plate 6 mm thick was mounted centrally so that any particle traversing the cloud chamber would have to penetrate this plate; the particles so doing would lose energy, and as a result of decreased velocity their track curvature would be greater on the side of emergence than on the side of incidence, thus affording a means of distinguishing between particles moving in opposite directions, and so enabling the charge sign to be determined.

Applying all the above criteria to the new tracks Anderson rejected the possibility of the chance occurrence of two independent electron tracks,—so placed as to merely give the appearance of one particle traversing the plate,—as an event not to be expected on the laws of probability.

That such a rejection was also justified was soon shown by the confirmatory work of other investigators; the first published being that done in Cambridge at the Cavendish Laboratory by Blackett and Occhialini. (3)

Many of their photographs showed groups of tracks proceeding from a common origin (usually just outside the chamber) and in these

groups some of the tracks of the same intensity of ionization and curvature were bent in one direction and some in the opposite one. In addition, from the study of the range (due to energy content) as well as the ionization effects, the possibility that those curved in the direction indicating a positive charge could be protons was definitely excluded so that in at least 14 cases there was doubt but that the tracks had been laid down by a particle similar to that discovered by Anderson and which was in fact the positron.

While such measurements could give only a rough estimate of the mass of the positron later work by the above mentioned authors as well as by Thibaud (4) indicated that the mass of electron and positron were the same within the limits of error of the methods used.

So much for the experimental proof of the existence of the positron, a particle of positively charged matter with a mass approximately the same as that of the electron.

What now is its origin? Millikan, in his cosmic ray work, had concluded long before the discovery of this positron, that cosmic rays were able to eject positive and negative particles from the nuclei of atoms. Whatever the mechanism of energy transformation or exchange, particles with energies as high as 6000 million electron volts (as measured by track curvature) have been found associated in pairs, as well as singly, in cloud chambers.

Modern nuclear theories, together with Einstein's mass-energy relation require that when there is a rearrangement in a system involving change of mass dm then the change in energy of the system dE be given by the equation $dE = c^2 dm$. Thus a nucleus considered as a tightly packed structure consisting of protons and neutrons constitutes a system in which the energy content is considerably less than that which the constituent parts of the nucleus would possess if they existed apart from their neighbors. This loss of energy which may be considered as that required to "pack" the particles together, can also be expressed in terms of mass defect, by the above relation due to Einstein, and from Aston's data, Rutherford gives the energy equivalent of the electronic mass at 511,000 electron volts. That of the proton corresponds to 940 million volts. Thus a proton of unit mass existing freely would lose energy,—decrease in mass,—by entering into combination with other protons necessary to produce the nucleus of heavier atoms. It would consequently emit energy in the process of such a formation. Conversely when the nucleus is struck by penetrating radiation (such as cosmic rays were considered to be) the impinging energy was conceived as being absorbed, and thus supplying the "energy of escape" whereby the tightly bound particles might be released from their bonds by having their mass defect supplied to them from without by the cosmic radiation.

Such an explanation could account for the high energy protons

found in cloud chamber,—and on the then excepted theory of the composition of the nucleus it would have also taken care of the electrons; but to explain the generation of electrons which do not exist in the nucleus as now understood, as well as to account for the presence of the positrons which previously needed no theory because they were unknown, we must appeal to Dirac's theory of electrons (5) and its application by Oppenheimer and Plesset (6) at the suggestion of Blackett and Occhialini (*loc.cit.*) to the formation of pairs of positive and negative electrons.

In this theory all but a few quantum states of negative kinetic energy are taken to be filled with negative electrons. There are a few states which are unoccupied and these behave like ordinary particles with positive kinetic energy and positive charge. Dirac originally wished to identify these "holes" with protons, but could not, as it was found by Weyl (*Gruppentheorie und Quantenmechanik* 2d. Ed. p. 234) that they must on theoretical grounds have the same mass as the electron. Without perhaps realizing it at the time Anderson had discovered the particle which fitted into the theoretical "hole" prepared for it by Dirac.

Not only has Dirac's theory of electron supplied a *raison d'être* for the positron but it also offers an answer to several other questions concerning this particle. Why for example did the positron so long evade discovery? Why does it not appear associated with matter under normal conditions? And what happens to the positrons which are formed by energy conversion processes in or near the nucleus subjected to penetrating radiation?

To understand the answers to these questions we must however refer to the fact that positrons are produced not only by cosmic radiation but also appear when hard gamma rays from radio-active substances are absorbed by matter. While the details of the experimental procedure involved in this type of production will be given later, the fact of such a positron source gave Oppenheimer and Plesset (*loc.cit.*) a starting point for their theory of pair production wherein they interpreted the absorption of gamma rays by atoms and the resulting production of pairs of oppositely charged electrons as a quasi-photo electric effect whereby the energy of the absorbed gamma quantum raises an electron from a negative energy state to a positive one, forming a "hole" or positron and a negatron. These authors after an analysis of conditions under which pair production may be occasioned conclude that in the case of a coulomb field the detection of pairs requires a radiation of energy greater than $2mc^2$, and thus show that if gamma rays of a specific energy above this value fall on a nucleus the formation of pairs is to be expected. Further application of this theory to a simple model gives the result that most of the kinetic energy available in the transformation is taken by the positives,—a point in good agreement with

experiment as will be seen later on. This greater energy of the positives may also be explained physically by the fact that the positive electron gains kinetic energy, and the negative loses it on escaping from the field of the nucleus.

Beck (7) considers the gamma ray absorption under discussion as a photoelectric absorption by "virtual electrons" i.e. those with negative kinetic energy, which are near the nucleus. These are considered to have a binding energy of $2mc^2$, and their effective number for any atom is proportional to the square of the atomic number. His theory also indicates that the birth process takes place within a distance $h/2mc \times 10^{-11}$ cm. of the nucleus; that is well inside the k shell.

Once a pair has been formed we must remember that the positive of this pair is really only an unoccupied energy state.

We might transfer the quaint idea of "nature abhorring a vacuum" from the realms of matter to that of energy, and thus perhaps be more easily convinced of the reasonableness of Dirac's subsequent explanation. For any free negative electron in the neighborhood of such an unoccupied energy state as is represented by the positron, will readily jump into that state, so filling up the "hole" and occasioning the simultaneous annihilation of itself and the positron, their masses being converted into radiant energy in the form of two quanta.

Dirac has calculated the probability of this annihilation process for a positron and finds that it depends on the number of extra nuclear electrons; and assuming an energy of 200 million electron volts as an upper limit and 100,000 as a lower limit he gives 0.36 as the integrated probability value for annihilation during the interval required for such a loss of energy by the positron. For energies lower than 100,000 e-volts the probability of annihilation in water per unit time reaches a constant value of 2.5×10^6 per second. So that positrons which live until they reach this energy will then die according to a probability law analogous to radioactive decay; their mean life in water being 3.6×10^{-10} second. Thus we see that the Dirac theory predicts a lifetime for the positron which is long enough for it to be observed in a cloud chamber but also short enough to explain why it has not been discovered by other methods.

This process of annihilation may take place in free space as described above with the emission of two quanta of total energy equal to $2m_0c^2$, and while these quanta must not necessarily be equal, that condition is the most probable energy distribution. If on the other hand the positron and electron are annihilated in a region so close to the nucleus that the recoil momentum can be absorbed by it, then the theory indicates that the whole mass $2m_0c^2$, of both positive and negative electron may be converted into a single quantum. This case however has never been unambiguously observed.

Let us return again to the experimental realm, to a time before the positron was discovered for the first evidence of positron annihilation. In 1930 Chao (8) undertook the study of scattered radiation, secondary radiation and absorption of secondary radiation in lead, using as his source a sharply defined beam of gamma rays from ThC". In addition to the Compton scattering he found a secondary radiation of 22.5 x units (55 million electron volts) and determined that this radiation was emitted in about the same intensity in a number of directions making angles of between 30° and 150° with the primary beam. He also found that this secondary radiation was stimulated only at a threshold value of the incident photons of roughly 2 million electron volts.

Gray and Tarrant in an elaborate series of experiments (9) not only confirmed Chao's findings but with further experimental refinements were able to correct for ionization due to extraneous sources as well as to estimate and allow for Compton scattering. As a result of their study of secondary radiation from C, O, K, Cu, Fe, Sn and Pb they found by absorption methods that this phenomena was produced in all the above named elements and that when these elements were irradiated with ThC" gamma rays in every case at least two-thirds of the secondary radiation was emitted in the form of a photon of energy of the order of half a million electron volts, the remaining third (in the case of lead) being in the form of higher energy radiation.

Their experiments also showed that the radiation first mentioned is isotropic in distribution at least within the angular range from 60° to 145° , within which arc as much as 65% of the absorbed energy has been observed as reradiated.

Upon calculating the fraction of total incident energy absorbed by the various elements Gray and Tarrant found that the increase was in the direction of increasing atomic number and that the nuclear interaction (i.e. the fraction of incident gamma ray energy which is reradiated from each nucleus) is roughly proportional to the square of the atomic number. These results when considered in the light of the annihilation theory of positrons seem to give that theory excellent support. For if we consider the half million electron volt secondary radiation alone we see that its energy is quite closely mc^2 . (where m is the electronic mass) Now if two electrons plus and minus, both with negligible kinetic energies combine, they must emit a radiation equal to their total rest mass $2mc^2$, and if this recombination takes place when both particles are free, the radiation will go off as two quanta each of the same energy, in order that conservation of both energy and momentum may be preserved. According to the quantitative agreement found above we may then explain the mechanism of the formation of secondary radiation in the following way: first a primary gamma quantum is absorbed forming a pair. The high velocity positron which is thus formed loses its kinetic energy by col-

lisions and at the end of its path combines with a free neighboring electron to form the secondary radiation, as a result of the double annihilation.

END OF PART I

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PHYSICAL CONSTANTS AT WESTON COLLEGE

REV. HENRY M. BROCK, S.J.

At the Georgetown Meeting of our Association in August 1934, a paper was read entitled "Laboratory Constants" of which an abstract appeared in the BULLETIN, Vol. XII, No. I. The desirability of having on record in the Laboratory of Physics certain important constants was pointed out and the methods of obtaining them were briefly indicated. As an illustration it may be of interest to give the values of the quantities discussed for Weston College.

Position: Cross on College Dome.

Latitude: $42^{\circ} 22' 57.08''$

Longitude: $71^{\circ} 19' 18.13''$ W.

$4^h 45^m 17.21^s$

Astronomical Observatory (Centre of Dome).

Latitude: $42^{\circ} 22' 54.2''$

$71^{\circ} 19' 21.9''$

$4^h 45^m 17.46^s$

Altitude above Mean Sea Level:

N.W. Corner of first platform of N stairs to Chapel.

227.526 feet.....69.350 meters,

Floor of Rotunda,

232.277 feet.....70.798 meters,

Floor of Physics Lecture Room (at entrance),

232.324 feet.....70.813 meters,

Acceleration of gravity ("g") in Physics Laboratory,
980.380 cms./secs.²

Magnetic declination (1935),

14° 54' west.

Magnetic inclination (1935)

73° 20'

Annual change negligible.

Horizontal component of intensity of earth's magnetism,

0.1650 dynes,

Annual decrease 0.0003 dynes.

The coordinates of the cross on the dome of the college were determined by triangulation according to the usual geodetic method as described in the BULLETIN, Vol. XI, No. 4; (May 1934). They are therefore geodetic and not astronomical. In the article the length of the known side of the triangle used—Prospect Hill, Lincoln Reservoir, Weston College—was omitted by an oversight. It was the distance between Prospect and Lincoln, viz. 6042.2 meters. From the checks obtained it would appear that the first decimal place for the seconds is correct with some uncertainty regarding the second place. The position of the Observatory was determined with reference to the dome.

A determination of our elevation was made about eight years ago by the following philosophers: Messrs. Carroll, Hauber, O'Callaghan and Sheehan, all of whom have been since ordained. They used our 15" Berger Wye Level and Gurley Rod with a special hand level to set the latter vertical. As the nearest precision bench mark then available was at the Newtonville station of the Boston & Albany R. R., which was rather too far away, they took for reference the elevation of the N.E. bolt at the base of block signal No. 169 on the Boston & Maine Railroad in the town of Lincoln. It is about 1.8 miles from the college. Within the past few years a certain number of precise bench marks have been established in our neighborhood, some by the Mass. Department of Public Works and others by the same department in collaboration with the U. S. Coast and Geodetic Survey. The elevation of the above mentioned bolt was compared with one of the state marks and found to be in error by nearly two feet. Investigation seemed to indicate that the value given to us the railroad referred rather to the corresponding bolt of an abandoned base near by.

A second determination of elevation was made in the fall of 1934 by the following philosophers, Messrs. Burns, Devlin, J. Donohue, F. Donohue, Eiardi, Fitzgerald, Langguth, Ring. They used the new bench mark, 2JB of the State and Geodetic Survey in Lincoln beside the east bound track of the Boston and Maine Railroad. It is east of Concord Road about 1.3 miles from the College. As before a line was run from the college to the mark and back according to the usual method. The two values for the difference in elevation agreed within 0.003 feet. The corresponding agreement in the first deter-

mination was 0.002 feet. The difference between the new value of the elevation of the rotunda floor and the old value corrected with reference to bench mark 2JB is 0.044 feet (0.54 inches). The new value has been adopted on account of the possibility of some change at the block signal in the course of seven years. The above elevations are given provisionally to three decimal places. They are still subject to a small correction when the adjusted value of the bench mark becomes available. The Geodetic Survey states that this will take some time on account of the very great amount of leveling done recently.

The values of the other constants were kindly furnished by the Geodetic Survey at Washington as we have no means of determining them here with satisfactory precision. The Director states that the value of the acceleration of gravity deduced from that of their nearest station at Cambridge, Mass., may be safely assumed to be correct within one in the second decimal place and probably within five in the third. Two of the magnetic elements are subject to annual change so that strictly speaking they are not constants. Their actual values can be obtained at any time by applying the proper correction. The magnetic declination and inclination are perhaps correct to five or more minutes for this region and the horizontal intensity to one or two thousandths of a dyne.



REFERENCES FOR ELECTRONICS

- Thermionic Emission, by Arnold L. Reiman. J. Wiley & Sons, N. Y. 1934.
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- New Theories of Photoelectricity, by L. A. DuBridge. Mermann & Co., Paris. 1935.
- The Cathode Ray Oscillograph in Radio Research, by R. A. Watson. H. M. Stationery Office, London.
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SEISMOLOGY

SEISMOLOGY NOTES

GEORGETOWN SEISMOLOGICAL OBSERVATORY, Rev. Frederick W. Sohon, S.J., Director of the Observatory, recently published two articles:

The Electrodynamic Ratio of the Galitzen Seismometer. Published: Transactions of the American Geophysical Union. Sixteenth Annual Meeting 1935.

A First Approximation of the Seismological Society of America. Published: Bulletin of the Seismological Society of America. Vol. 25, No. 4, October 1935.

Reprints may be had at the Georgetown Observatory.

WESTON SEISMOLOGICAL OBSERVATORY. In the recent *status* Fr. George A. O'Donnell, S.J., was appointed Director of the Observatory replacing Fr. Henry M. Brock, S.J., who has held this office since 1929.

Boston and vicinity has become quite seismic-minded since the past summer. One motive for this was the presentation of a fund to Fr. M. J. Ahern, S.J., on October 20th, by friends, which was to be expended in installing a Seismological Observatory of the First Order. As soon as installation has been effected, a more detailed account will appear in the Bulletin. The Press carried many glowing tributes to Dr. Ahern and many accounts of this presentation and the importance of seismological research. Naturally, those interested in this branch of science are very grateful to Fr. Ahern for the opportunity for added research which he has made possible.

Following this was the Canadian quake of the morning of November 1st. The Boston district was sufficiently within the zone to have people awakened, pictures knocked from the wall and merry-makers returning from Hallowe'en parties see tall buildings sway. Weston College was phoned by the Press about 1:15 A. M. (E.S.T.) for information. Our seismologists were therefore able to witness the coda still recording. Within the hour sufficient information concerning distance and intensity was given the papers. The worried populace were thus calmed by breakfast time when they read that

the temblor while severe, was about 400 miles away. That afternoon, working with Dr. Don Leet of the Harvard Seismic Station, a provisional epicenter was placed near Doucet, Quebec. The Harvard instruments proved a little too sensitive for this quake and not much more than the initial phase could be ascertained at the time, while the 80 k. Wiechert at Weston with its lower magnification gave grams on both components that were quite readable.

Visitors to the Observatory during the past few weeks included Drs. Shapley, Mather and Leet of Harvard University, Dr. McComb of the Coast and Geodetic Survey, and Dr. Shea of Mass. Institute of Technology.

THE JESUIT SEISMOLOGICAL EXHIBIT AT ST. LOUIS

Part of the Annual Science Exhibition of the American Association for the Advancement of Science held in the St. Louis Municipal Auditorium was a Jesuit Seismological Exhibit occupying two large booths. A map of the United States extending almost the length of the exhibit but raised high enough so as not to be obscured by other parts of the exhibit, formed the background of the Jesuit Display. The position of the Jesuit Stations, the Government Stations and the other Independent Stations of the United States and Canada were shown on the map by colored lights flashed intermittently like a Times Square electric sign. The actual stations, their equipment and their personnel were shown in a series of photographs arranged around the walls of the booths. The work of the Stations was shown by a chain of exhibits from the various stations, each exhibit being a distinct but connected link in the chain. This map was constructed at the St. Louis University Seismological station.

Fordham University devised a working model (using a discarded Pin Ball Game) to show an earthquake in the making. The visitor to the booth produced the earthquake by causing a steel ball to fall into an earthquake area marked on a map of the world forming the upper part of the machine. The middle part of the machine then showed how an earthquake travelled through the earth. Colored lights enabled the observer to follow the paths of the compressional and transverse waves—both direct and reflected, through the earth to the observatory. Here a pendulum was set up and as the waves passed beneath it, they caused the pendulum to vibrate and produce their visible record on an illuminated film.

Canisius College showed an enlarged working model of the Seismograph itself, showing very clearly how the principle of inertia was used to record the waves and how the mechanical lever was used to magnify their record. Next came the Exhibit of Weston College to show the latest type of actual Seismographs—the Benioff vertical instrument. It really formed the pivot of the exhibit and though not

set up in actual operation, it recorded a large number of gasps and tremors of amazement at its wonderful workmanship. Georgetown and Xavier showed the finished work of an actual seismograph. The former showed a complete year's record of microseisms, while the latter gave a similar display of macroseisms, the chief feature of each quake being noted on the record in bold type.

Then finally St. Louis University showed some of the concrete conclusions drawn from these records in the form of a large plaster model of the earth, showing its internal structure as determined by earthquake records. Two of the prominent discontinuities or layers shown in the model were discovered by and named after two Jesuit workers, Father Repetti of the Maryland-New York Province, and Mr. Dahm, a lay graduate of St. Louis University and now an assistant there.

Pamphlets explaining the elementary principles of seismology were distributed gratis at the Exhibit to some four thousand people. Quite a number of these (the pamphlets, not the people) are still on hand at Fordham. If any readers of the Bulletin would care for copies, they may secure them by sending a postal.



NEWS ITEMS

The American Association For The Advancement of Science

The annual meeting of the American Association for the Advancement of Science was held at St. Louis, Mo., from December 30 to January 4th.

The Catholic Round Table of Science meeting was conducted at St. Louis University Medical School on January 2nd. Rev. Alphonse M. Schwitalla, S.J., was host to one hundred and forty members of the Catholic Round Table of Science at a luncheon served in the cafeteria of the Medical School Building.

After luncheon the regular meeting was conducted by Father Schwitalla and Father Anselm Keefe. Among the topics for discussion were: In how far is academic status dependent upon faculty research; proportions of time allotted to faculty research and other duties; students' and research careers.

The Jesuits attending the meeting of the A. A. A. S., held a special meeting at St. Louis University in the Administration Building, on January 2nd. This meeting was called at the request of V. Rev. Daniel M. O'Connell, S.J., Commissioner of Education for the American Assistancy. Rev. Richard B. Schmitt, S.J., of Loyola College, Baltimore, Maryland, presided. The topics for discussion were: Should we attempt sectional conventions, and a national convention? Should we attend secular conventions and appear on their programs? Should we attempt a National Science Magazine? Should we attempt an Institute of Science?

Father James B. Macelwane was elected Chairman, and Father E. Kolkmeier was elected Secretary, for the next meeting.

The Jesuit Seismological Association had an excellent exhibit in the St. Louis Auditorium during the A. A. A. S. convention. The work of the Society in the field of Seismology was excellently portrayed. A leaflet on "Earthquakes" was distributed and was in great demand. Father James B. Macelwane of St. Louis University and Father J. Joseph Lynch of Fordham University were in constant attendance and answered many questions about seismographs and earthquakes.

Georgetown University, Mathematics Department

The first meeting of the Angelo Secchi Academy of Georgetown University was held on November 20, 1935, at eight o'clock in the evening. There were about twenty persons present. The discussion was opened by the reading of a paper by Father Sohon entitled "The Concept of Order". In the debate that followed, Dr. Solterer, of the Department of Economics, challenged the applicability of the statistical methods to physical chemistry without certain restrictions, and Mr. Howard took up arms in defense of his own field. The debate then began to turn on the point of the degree of certitude attainable in the physical sciences, and a number of others joined the disputants, including Mr. Schweder.

Loyola College, Baltimore, Maryland, Chemistry Department

On February 7th, Dr. E. Emmet Reid of Johns Hopkins University, Director of Organic Research, lectured to the members of the Loyola Chemists' Club. The subject: "The Melting Points of Series of Organic Compounds." The large attendance gave evidence of the popularity of the speaker; this is the third lecture given by Dr. Reid to the Chemists' Club.

Another guest speaker of the Chemistry Department was Dr. E. Gaston Vanden Bosche, Assistant Professor of Inorganic Chemistry at the University of Maryland. The lecturer gave a most interesting talk on the subject: "Dental Alloys, Old and New."

Boston College, New England Chapter of the C. R. T. S.

The first meeting of the New England Chapter of the Catholic Round Table of Science was held at Boston College on January 26th. Seventy-five professors of Science from New England colleges and universities were present. Rev. John A. Tobin, S.J., Professor of Physics at Boston College, presided at the meeting. Luncheon was served in the Science Building. After luncheon, various topics of research problems were discussed, similar to those discussed at the St. Louis meeting.

The members voted to hold two meetings a year. The next meeting will be held in April at Holy Cross College, Worcester, Mass. Father Tobin was appointed Secretary of the New England Chapter.

After the meeting, the visiting professors made a tour of inspection of the various laboratories of Boston College. Nine of the Catholic Colleges were represented, and five non-catholic colleges sent representatives.

Other chapters of the C. R. T. S. have been organized in New York City and Buffalo.

Gonzaga University, Spokane, Washington

Physics Department

The American Association of Jesuit Scientists, North-Western Section, held their annual meeting at Gonzaga University during the last week in December. The principle address was given by Rev. Francis J. Altman, S.J., on the subject of Seismology.

Mr. Thomas Walsh, a graduate of St. Mary's College, Kansas, Mo., and the builder of Coulee Dam, the largest structure ever built by man, invited the Gonzaga University Engineering School to visit and inspect the project.

The Engineering Department at Gonzaga University is now fully equipped for a four-years' course in electrical engineering.



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